

SCIENCE

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THE COMPARATIVE METHOD OF STUDY.¹

BY GEORGE H. JOHNSON, SC.D., ST. LOUIS, MO.

WE are all acquainted with the word evolution as used in natural history to express the doctrine that many different forms of animal and vegetable life have been derived from a common ancestry. The use of the word in a more general sense in astronomy and sociology is almost as well known. Philosophers, historians, and inventors, as well as naturalists, have discovered in the present generation that evolution, defined simply as the theory which recognizes not only the formal and external connection and correspondence of entities and events, but also the causal connection,—so far as that may exist,—is not only true but is also exceedingly useful and fruitful as a method to guide thought and investigation.

If we would make the most rapid progress on the current of modern thought, we must be in the current and know its trend, and to determine its trend we must know both its present condition and the previous conditions through which it passed to reach the present; just as in determining the direction of a river current we must use floats to show the path traced by the surrounding water, and in determining a comet's orbit we must use several positions to compute its future course, so in all subjects which involve human personality and social progress there are certain well-determined positions formerly occupied, which, compared with the present, will indicate the future.

A good historian, for example, is not satisfied with his narrative until he has shown the events in their proper relation to each other. Indeed, the true scholar, the man of profound mind and practical learning, is not he who has apprehended and remembered the greatest number of great and useful facts; it is he who has a systematic, correlated knowledge of facts. Let us consider one or two illustrations.

In some sciences it seems as if human thought moved in a kind of cycle, so that by going back far enough a period or phase in the history of the science or doctrine will be found which is very much like its present condition, or like any other condition which we may be considering. Even back to the time of Solomon, we have his authority for the proverb that there is nothing new under the sun, and if it was true then it must be true more particularly at the present day. This does not teach that there is no real progress in human knowledge, nor that there are no new combinations of public events such as constitute history. But it is evident that all motion and change in human thought, all changes in national life, all movements in literature, art, and even science, are not progressive. Indeed, there is very much intellectual motion to only a little progress. In this respect the human mind may be compared to the limbs of a child. A little girl, when she is excited and hurried, will jump up and down and make very little forward motion, notwithstanding—or rather because of—her eager desire to do so. She wastes a large amount of energy simply in crossing a room. Her movements are not properly co-ordinated; she does not know what muscles to use in order to attain the desired position. How true this is of the intellectual movements of men!

A man in his anxiety to get ahead rapidly in the world, to acquire more money, to get some fame or honor, position or power, will make many senseless and useless actions which do not at all hasten the attainment in view. We all do it more or less; a man would be supremely wise or fortunate who was not occasionally humiliated by discovering that his most carefully laid plans and most deliberate actions had very unforeseen effects.

¹ Abstract of a paper written for a popular audience.

To regard the world as a stage and all the rest of mankind as players is an instructive as well as amusing way of contemplating our fellowmen. To see politicians standing in a high place and reaching forward and upward to grasp more power and influence, and in their eager and irrational motions losing their foothold and falling prostrate, is a spectacle which is constantly before our eyes, at least the reaching is constantly before our eyes and the falling is periodically manifest at election time.

But the same disposition is seen, and similar accidents happen, to men in all walks of life. That is to say, that the child who is learning to walk, and the man who is learning to control himself and other men, go through many motions and actions which are not really helpful for the purpose in view. But these misguided actions are educational in their results, so that if a man lives long enough he is likely to become measurably wise by virtue of his own mistakes.

Men often act with very little sound theory or experience to guide them. In many respects we are like a man blindfolded, who hears some noise but is doubtful from whence it comes; if he has the opportunity to explore the room in all directions he is likely to find the right corner before long, even though his hearing at first directly misleads him. And so in all human knowledge, if we only have opportunities to make enough mistakes we are likely to have ultimately some measure of successful intellectual progress. We need not experiment with what is dangerous, nor carelessly make mistakes, but when we have no sure knowledge to guide us we should learn as much as possible from the experiments made by ourselves and others; thus, proving all things, we may hold fast to that which is good.

Very much of vaunted human progress is at best only up along the arc of a helix, so that there is much motion to only a little rise; and, after making a complete turn or cycle, society is found just where it was before, except that it is a little higher in experience and therefore enjoys a wider horizon and clearer view. As mountain railroads wind in grand loops and horse-shoe curves around the valleys and up to the passes, making miles of road to gain a few hundred feet in altitude, so human knowledge moves in a kind of cycle such that it is possible to stop at any point and look back and down at the corresponding point in time past.

Does any heretical doctrine arise in the Church? The expert in ecclesiastical history will have no difficulty in telling us when the same heresy long ago produced a similar dissension, and quite likely he will be able to show that again and again the same doctrine in only slightly different form has been the ground for divisions in the Church. Where several of these recurring phases can be definitely located it might be possible to investigate the law of their recurrence or, as the physicist would say, to investigate the period of the vibration or undulation. The weather prophets, who employ themselves and amuse the public by predicting when we shall have the coldest day of winter and the hottest day of summer, might, by the use of some statistics and mathematics, derive formulas designed to express the periods of moral, social, and intellectual movements.

One reason why such formulas are impracticable is because they would contain an infinite number of terms; that is to say, the time of recurrence of any sociological phenomenon is a function of an infinite number of conditions. But most of these conditions are insignificant compared with a few of the most important ones, and so we could apply to this problem the method of finding approximately the value of an infinite but rapidly converging series.

For example, consider a financial panic, which is never the result of a single cause. At certain times the failure of a great bank is enough to precipitate a widespread and disastrous panic;

at other times, when the wave of public confidence is high and strong, such a failure has no perceptible effect beyond those immediately concerned. To predict the time of the next panic it would be necessary first to determine the periodic laws of speculative eras, expansion and contraction of the currency, over production, and other principal causes, and then combine them to find when like phases coincide. Just as two rays of light of opposite phases may by interference annihilate each other, so two social movements or tendencies, both of great power and effect, may, when they enter as terms in the formula of another movement, cancel each other by reason of their opposite signs or phases. On the contrary, all of the greatest movements of the social world, such as the founding and spread of Christianity, the fall of Rome, the Reformation, the colonization of America, and the French Revolution, have been the result of the synchronous combinations of many causes or terms of the same sign and phase, so that the sum of the whole—even if we neglect the infinite number of small terms—is one of transcendent magnitude.

The great advantage of the philosophical study of history is that by this method the constituent elements of events and the movements to which they belong are made apparent, and for this purpose we must be provided with the data for expressing the trend and phase of all the political, philosophical, and religious movements to which they are related. The complexity of the problems involved is indicated by the fact that different scholars arrive at such contradictory conclusions. Nevertheless, it is believed that this modern method of investigation will revolutionize all the social sciences as it has already revolutionized political economy, and that after the method has been more systematically applied to modern statistics, and the number of terms considered has been increased, the conclusions or results reached by different authorities will be less and less discrepant, and that thus we may hope ultimately to reach a certainty and precision, in the social and metaphysical sciences, which will be comparable to the precision of physical data. If we throw a pebble into the air we can express mathematically the motion of the earth toward the pebble as well as the motion of the pebble toward the earth, and we might perhaps express in a similar way the effect which the repair of a roof in San Francisco would have upon the prices of building materials in New York, and *vice versa*. More than this, is it not possible that a new psychology will be able to weigh and measure the volitions, tastes, and emotions of the mind, so that this science as well as history and political economy may become partly quantitative?

The methods of mathematics can be applied to the metaphysical sciences more extensively than has been done heretofore. These methods have already been applied, in a limited way, to all subjects having much statistical data, also to logic. Indeed, mathematical forms and analysis may be used in any science, as chemistry, which is subject to quantitative treatment. The qualitative analysis must always precede the quantitative analysis in any science, but most subjects are now so fully developed that it is time for original research to be directed to the quantitative treatment. This is being done in a kind of tentative way at several universities, and it is believed that the comparative, quantitative method of investigation will be as useful in other sciences as it has already proved to be in political economy and philosophy.

NATURAL SELECTION AND USE-INHERITANCE.

BY VICTOR YARROS, BOSTON, MASS.

EVOLUTIONISTS will be extremely gratified to learn that Mr. Spencer has resumed the discussion of the subject of the factors of organic evolution. Since the publication, several years ago, of Mr. Spencer's controversial essays on this subject, the so-called pure-Darwinians have practically enjoyed a monopoly of the field; and some of the more rash biologists have even allowed themselves to advance the claim that the use inheritance hypothesis was utterly discredited. Mr. Spencer's unsatisfactory state of health, it was understood, necessitated his neglect of this and many other "unsettled problems" and the concentration of his atten-

tion on ethical questions,—the part of his synthetic philosophy rightly regarded by all as the crown of the whole. Students of evolution were anxious to hear "the other side," the answers to the formidable objections of Professor Weissman and his disciples or co-believers, and the announcement of Dr. Romanes's "Darwin and After Darwin," a part of which work was to treat elaborately the question of the number and relative importance of the factors of organic evolution, was received with great pleasure. But no one realizes more keenly the transcendent importance of the question of the inheritance of acquired characters than Mr. Spencer, and he is to be congratulated upon the kindness of fortune that has enabled him to spare some time and energy to the further consideration of the subject, use-inheritance *vs.* sexual selection. It will conduce to firmness of grasp and clearness of understanding to quote here certain passages from Mr. Spencer's preface to his "Factors of Organic Evolution."

"Though mental phenomena of many kinds," wrote Mr. Spencer, "and especially of the simpler kinds, are explicable only as resulting from the natural selection of favorable variations; yet there are, I believe, still more numerous mental phenomena, including all those of any considerable complexity, which cannot be explained otherwise than as results of the inheritance of functionally-produced modifications. What theory of psychological evolution is espoused, thus depends on acceptance or rejection of the doctrine that not only in the individual, but in the successions of individuals, use and disuse of parts produce respectively increase and decrease of them.

"Of course there are involved the conceptions we form of the genesis and nature of our higher emotions; and, by implication, the conceptions we form of our moral intuitions. If functionally-produced modifications are inheritable, then the mental associations habitually produced in individuals by experiences of the relations between actions and their consequences, pleasurable or painful, may, in the successions of individuals, generate innate tendencies to like or dislike such actions. But, if not, the genesis of such tendencies is, as we shall see, not satisfactorily explicable.

"That our sociological beliefs must also be profoundly affected by the conclusions we draw on this point, is obvious. If a nation is modified *en masse* by transmission of the effects produced on the natures of its members by those modes of daily activity which its institutions and circumstances involve, then we must infer that such institutions and circumstances mould its members far more rapidly and comprehensively than they do if the sole cause of adaptation to them is the more frequent survival of individuals who happen to have varied in favorable ways."

The above expresses Mr. Spencer's view of the profound importance of the indirect bearings of the purely biological argument upon the factors of organic evolution. Now that we have refreshed our memory on this point, let us proceed to give a brief but careful summary of Mr. Spencer's latest contribution to the controversy, to be found in an article, entitled "On the Inadequacy of Natural Selection," in the *Contemporary Review* for February. We preserve as far as possible Mr. Spencer's style.

Students of psychology are familiar with the experiments of Weber on the sense of touch. He found that different parts of the surface differ widely in their ability to give information concerning the things touched. By actual measurements he showed that the end of the forefinger has thirty times the tactual discriminativeness which the middle of the back has. Between these extremes there are gradations. The inner surfaces of the second joints of the finger can distinguish separateness of positions only half as well as the tip of the forefinger. The innermost joints are still less discriminating, their power being equal to that of the tip of the nose. The palm of the hand and the cheek have alike one fifth of the perceptiveness which the tip of the forefinger has, and the lower part of the forehead has one-half of that possessed by the cheek. The crown of the head is far less discriminating, and the breast still less.

What is the meaning of these differences? How, in the course of evolution, have they been established? If "natural selection" or survival of the fittest is the assigned cause, then it is required to show in what way each of these degrees of endowment has

advantaged the possessor to such extent that not infrequently life has been preserved by it. It is reasonable to assume that the parts have not become so widely unlike in perceptiveness without some cause, and, if the cause alleged is natural selection, it becomes necessary to show that the greater degree of the power possessed by this part than by that has conduced so much to the maintenance of life that an individual in whom a variation had produced better adjustment to needs, thereby maintained life when some others lost it, and that among the descendants inheriting this variation there was a derived advantage such as enabled them to multiply more than the descendants of individuals not possessing it. Can anything like this be shown?

That the superior perceptiveness of the forefinger-tip has thus arisen, might be contended with some apparent reason, as such perceptiveness is an important aid to manipulation. But how about the back of the trunk and its face, or the tip of the nose, or the thigh? The survival of the fittest cannot explain these differences of perceptiveness. But if there has been in operation a cause which it is now the fashion to deny, the various differences are at once accounted for. This cause is the transmission of inherited traits or characters.

(Here Mr. Spencer records some experiments which show that constant exercise of the tactual nervous structures leads to further development, to greater discriminativeness. The perceptiveness of the finger-ends of the blind who read from raised letters and of compositors is greater than that of the finger-ends of other people.)

Now, if acquired structural traits are inheritable, the gradations in tactual perceptiveness are the result of the gradations in the tactual exercises of the parts. The trunk has but little converse with external bodies, and it has but small discriminative power; what power it has is greater on its face than on its back, corresponding to the fact that the chest and abdomen are more frequently explored by the hands, this difference being probably in part inherited from inferior creatures. The middle of the forearm and the middle of the thigh are obtuse, having rare experience of irregular foreign bodies. The tip of the nose has considerable tactual experience, hence its greater perceptiveness. The inner surfaces of the hands are more constantly occupied in touching than are the back of the hand, breast, forearm, forehead, while the tips of the fingers come into play not only when things are grasped, but when things are felt at or manipulated. If then it be that the extra perceptiveness acquired from extra tactual activities, as in a compositor, is inheritable, the gradations of tactual perceptiveness are explained.

The tip of the tongue exceeds all other parts in power of tactual discrimination; why such perceptiveness? Its functions of moving food during mastication and of making many of the articulations constituting speech, are not materially aided by extreme perceptiveness, and natural selection cannot have caused it. But assume inheritance of acquired traits, and there is no difficulty, for the tongue-tip has, above all other parts of the body, increasing experiences of small irregularities of surface. It is in contact with the teeth, and either consciously or unconsciously is continually exploring them. There is hardly a moment in which impressions of adjacent but different portions are not being yielded to it by either the surfaces of the teeth or their edges. No advantage is gained; it is simply that the tongue's position renders perpetual exploration almost inevitable; and by perpetual exploration is developed this unique power of discrimination.

Thus the law holds throughout, from this highest degree of perceptiveness of the tongue-tip to its lowest degree on the back of the trunk; and no other explanation of the facts seems possible.

But some biologists might contend that *panmixia* affords an adequate explanation of the facts. So Mr. Spencer, after pointing out that the explanation by *panmixia* implies that these gradations of perceptiveness have been arrived at by the dwindling of nervous structures, and hence makes an unproved and improbable assumption the basis of the argument, proceeds to establish that, even with this objection passed over, it may with certainty be denied that *panmixia* can furnish an explanation. As this part of the essay is left unfinished, it would be unwise to attempt an

abstract of the Spencerian criticism of the *panmixia* explanation. We shall return to the subject as soon as Mr. Spencer brings his argument to a close.

FEEDING-LINES OF A LIVING LAND GASTEROPOD ON LICHENED SLATE.

BY J. B. WOODWORTH, SOMERVILLE, MASS.

IN searching for fossils in the Carboniferous rocks of Attleboro, Mass., about three years ago, I found on the surface of a vertical stratum of micaceous slaty sandstone, in an old quarry, what at first glance appeared to be annelid trails resembling the form known as *Nereites* common in the Silurian. Further examination showed me at once, however, that these markings were caused by the gnawing away of a drab-colored crust of lichens and dust which concealed the real appearance of the rock. The trails were in the form of bands about one-quarter of an inch wide, wandering over the surface of the outcrop, or curved back and forth on each other, so as to approach but rarely cross. These bands or trails were made up of a series of crescentic cross-markings united alternately right and left with the next adjacent in the series so as to form a continuous, closely pressed, sigmoid line, which in itself constituted the whole of the trail. The trail was evidently the feeding-line of some animal. Another occurrence which I have more recently observed in Bristol County, Mass., exhibited a trace of slime along the feeding-line, such as is left by slugs or land snails, thus showing that the feeder was probably a gasteropod.

Ebenezer Emmons, in the *Agriculture of New York*, Vol. I., 1846, p. 68, describes a trail found upon the surface of the fine green slate of Salem, Washington Co., N. Y., included in his "Taconic System," to which he gave the name *Nemopodia tenuissima*. The figure of this trail on pl. 14, fig. 1, of that work, agrees closely with the Attleboro trails. In an explanatory note, p. 365, Emmons states that this trail has been shown, he thinks, satisfactorily by his friend Dr. Fitch, "to be formed by some living unknown animal." It seems to me highly probable that the trail observed by Emmons, and shown to be not a fossil by his friend Dr. Fitch, was also that of a gasteropod. Conchologists may be familiar with the animal which makes these tracks, if I am right in thinking that they are made by gasteropods at all. As yet I have been unable to catch the animal at its work.

NOTE ON THE GENERIC NAME CHIROTES.

BY LEONHARD STEJNEGER, CURATOR DEPT. REPT. AND BATR., U. S. NAT. MUSEUM, WASHINGTON, D. C.

THE application of the law of priority necessitates the abolition of Cuvier's name *Chirotes* for the "Two-handed Ground Worm." No less than three generic names, formally proposed and diagnosed, have priority over *Chirotes*, none of which is pre-occupied, and which in turn would have to be adopted, should any of the older ones for some reason become unavailable.

Bonnaterre seems to have been the first to give a Latin name to La Cèpede's Cannelée, and to recognize its generic distinctness. However, by sheer carelessness he neglected to do so and a solitary "B" stands for the generic name he intended to impose. It may be assumed that he meant to call it *Bipes*, but we have nothing to do with assumptions. At the same time he included as the second species of his intended genus, Pallas's *Lacerta apus*, under the name *B. sheltopusik*.

Latreille, however, saw the incongruity of uniting the two in the same genus, and expressly restricted the name *Bipes* to the *B. canaliculatus*. The genus was thus formally established, named, diagnosed and restricted in 1802 as *Bipes*. Bonnaterre's other species he made a separate genus, *Sheltopusik*,² renaming Pallas's species *Sheltopusik didactylus*.³ The latter will therefore stand as *Sheltopusik apus* (Pall.). It will be observed that this

¹ "Nous ne connaissons encore qu'une seule espèce bien distincte de ce genre."

² Latreille, Hist. Nat. Rept., II., 1802, p. 271.

³ Latreille, tom. cit., p. 273.

generic appellation antedates Daudin's *Ophisaurus*, of the type which is our glass-snake, *O. ventralis*. Boulenger regards both species as congeneric, and if he respects the law of priority he will have to call the latter *Sheltopusik ventralis*. It is hardly probable, however, that any American herpetologist will follow him in placing the Eurasiatic species with two posterior legs in the same genus as the totally legless North American species.

According to the above there can be no doubt but that the generic name for the *Chirotes* must stand as *Bipes*, and the family name will, accordingly, be *Bipedidae*. The synonymy of the genus is as follows:—

Bipes LATREILLE.

1802.—*Bipes* LATREILLE, Hist. Nat. Rept., II., p. 90 (type *B. canaliculatus*).

1804.—*Microdipus* HERMANN, Obs. Zool., p. 289 (same type).

1811.—*Bimannus* OPPEL, Ord. Rept., p. 45 (same type).

1817.—*Chirotes* CUVIER, Regne Anim., 1st ed., II., p. 57 (same type).

Species: *Bipes canaliculatus* Bonnaterrre.

THE RAVAGES OF BOOK WORMS.

At a meeting of the Massachusetts Historical Society, held Feb. 9, 1893, Dr. Samuel A. Green, after showing two volumes that had been completely riddled by the ravages of insects, as well as some specimens of the animals in various stages, made the following remarks:—

For a long period of years I have been looking for living specimens of the so called "book-worm," of which traces are occasionally found in old volumes; and I was expecting to find an invertebrate animal of the class of annelides. In this library at the present time there are books perforated with clean-cut holes opening into sinuous cavities, which usually run up the back of the volumes, and sometimes perforate the leather covers and the body of the book; but I have never detected the live culprit that does the mischief. For the most part the injury is confined to such as are bound in leather, and the ravages of the insect appear to depend on its hunger. The external orifices look like so many shot-holes, but the channels are anything but straight. From a long examination of the subject I am inclined to think that all the damage was done before the library came to this site in the spring of 1838. At all events, there is no reason to suppose that any of the mischief has been caused during the last fifty years. Perhaps the furnace heat dries up the moisture which is a requisite condition for the life and propagation of the little animal.

Nearly two years ago I received a parcel of books from Florida, of which some were infested with vermin, and more or less perforated in the manner I have described. It occurred to me that they would make a good breeding-farm and experiment station for learning the habits of the insect; and I accordingly sent several of the volumes to my friend Mr. Samuel Garman, who is connected with the Museum of Comparative Zoölogy at Cambridge, for his care and observation. From him I learn that the principal offender is an animal known popularly as the Buffalo Bug, though he is helped in his work by kindred spirits, not allied to him according to the rules of natural history. Mr. Garman's letter gives the result of his labors so fully as to leave nothing to be desired, and is as follows:—

MUSEUM OF COMPARATIVE ZOÖLOGY, CAMBRIDGE, MASS.,
Feb. 7, 1893.

DR. SAMUEL A. GREEN, BOSTON, MASS.

Sir:—The infested books sent for examination to this Museum, through the kindness of Mr. George E. Littlefield, were received July 15, 1891. They were inspected and, containing individuals of a couple of species of living insects, were at once enclosed in glass for further developments. A year afterward live specimens of both kinds were still at work. Besides those that reached us alive, a third species had left traces of former presence in a number of empty egg-cases.

Five of the volumes were bound in cloth. On these the principal damage appeared at the edges, which were eaten away and

disfigured by large burrows extending inward. Two volumes were bound in leather. The edges of these were not so much disturbed; but numerous perforations, somewhat like shot-holes externally, passed through the leather, enlarging and ramifying in the interior. As if made by smaller insects, the sides of these holes were neater and cleaner cuttings than those in the burrows on the edges of the other volumes.

The insects were all identified as well-known enemies of libraries, cabinets, and wardrobes. One of them is a species of what are commonly designated "fish bugs," "silver fish," "bristle tails," etc. By entomologists they are called *Lepisma*; the species in hand is probably *Lepisma saccharina*. It is a small, elongate, silvery, very active creature, frequently discovered under objects, or between the leaves of books, whence it escapes by its extraordinary quickness of movement. Paste and the sizing or enamel of some kinds of paper are very attractive to it. In some cases it eats off the entire surface of the sheet, including the ink, without making perforations; in others the leaves are completely destroyed. The last specimen of this insect in these books was killed Feb. 5, 1893, which proves the species to be sufficiently at home in this latitude.

The second of the three is one of the "Buffalo Bugs," or "Carpet Bugs," so called; not really bugs, but beetles. The species before us is the *Anthrenus varius* of scientists, very common in Boston and Cambridge, as in other portions of the temperate regions and the tropics. Very likely the "shot-holes" in the leather-bound volumes are of its making, though it may have been aided in the deeper and larger chambers by one or both of the others. The damage done by this insect in the house, museum, and library is too well known to call for further comment. Living individuals were taken from the books nearly a year after they were isolated.

The third species had disappeared before the arrival of the books, leaving only its burrows, excrement, and empty egg-cases, which, however, leave no doubt of the identity of the animal with one of the cockroaches, possibly the species *Blatta Australasica*. The cases agree in size with those of *Blatta Americana*, but have thirteen impressions on each side, as if the number of eggs were twenty-six. The ravages of the cockroaches are greatest in the tropics, but some of the species range through the temperate zones and even northward. An extract from Westwood and Drury will serve to indicate the character of their work:—

"They devour all kinds of victuals, dressed and undressed, and damage all sorts of clothing, leather, books, paper, etc., which, if they do not destroy, at least they soil, as they frequently deposit a drop of their excrement where they settle. They swarm by myriads in old houses, making every part filthy beyond description. They have also the power of making a noise like a sharp knocking with the knuckle upon the wainscoting; *Blatta gigantea* being thence known to the West Indies by the name of drummer; and this they keep up, replying to each other, throughout the night; moreover, they attack sleeping persons, and will even eat the extremities of the dead."

This quotation makes it appear that authors as well as books are endangered by this outlaw. With energies exclusively turned against properly selected examples of both, what a world of good it might do mankind! The discrimination lacking, the insect must be treated as a common enemy. As a bane for "silver fish" and cockroaches, pyrethrum insect powder is said to be effectual. For a number of years I have used, on *lepisma* and roach, a mixture containing phosphorus, "The Infalible Water Bug and Roach Exterminator," made by Barnard & Co., 7 Temple Place, Boston, and, without other interest in advertising the compound, have found it entirely satisfactory in its effects. Bisulphide carbon, evaporated in closed boxes or cases containing the infested articles, is used to do away with the "Buffalo Bugs."

Very respectfully yours,

SAMUEL GARMAN.

MR. FREDERICK VERNON COVILLE has been appointed botanist to the Department of Agriculture in place of Dr. George Vasey, deceased. Mr. Coville has been for some years past one of Dr. Vasey's assistants.

CURRENT NOTES ON ANTHROPOLOGY.—XXV.

[Edited by D. G. Brinton, M.D., LL.D.]

Points in Chinese Ethnology.

DR. GUSTAVE SCHLEGEL, who is Professor of Chinese Literature in the University of Leyden, has undertaken to resolve a series of problems relating to the identification of various mysterious peoples mentioned by the early Chinese historians. They have more than special interest, because they bear on the question of the peopling of America from Asiatic sources.

As early as 500 A.D., there is a description of a tattooed people, *Wen-chin*, living 7000 *li* northeast of Japan. Dr. Schlegel identifies them with the inhabitants of Ouroup, one of the Kurile islands; but adds that, in historic times, every tribe from the island of Yezo to Greenland had the habit of tattooing, except the Ghiliaks and Itulmens of Kamschatka. He would also place the "Land of Women," *Niu-kouo*, said to be 1,000 *li* east of Fusan, somewhere in the southern portion of the Kurile Archipelago. In an article on "The Land of Little Men," he maintains the important thesis that the Tungusic stock at one time occupied the whole of the Japanese archipelago. Professor Schlegel's essays may be had of E. J. Brill, publisher, Leyden.

An interesting study of Chinese gilds recently appeared in the *Yale Review*, from the pen of the accomplished sinologue, Mr. Frederick Wells Williams. Within a few pages he exemplifies the great extent and completeness of the gild system among the Chinese, and illustrates the singular similarity of their laws to those of mediæval and modern trades unions in Europe and America.

Progress in American Archæology.

The science of American archæology and ethnology owes a large and increasing debt to Professor F. W. Putnam, who represents those branches in the faculty of Harvard University, and who is also Curator of the Peabody Museum, and Chief of the Department of Ethnology and Archæology of the World's Columbian Exposition. All these posts he fills admirably, as any one will see who will read his Report of the Peabody Museum for 1892, just issued. One fact will be sufficient: that within the last two years he has engaged, trained, and sent into the field — and the field means the whole American continent, from Greenland to Tierra del Fuego — about one hundred assistants and students, actively interested in collecting archæological and ethnological material. He says with pardonable pride and entire justice: "Never before has such an extensive field of anthropological research been covered in two years' time." A brief reference to the results obtained is included in the report. Naturally, the exploration of the wonderful ruins of Copan, Honduras, is most prominently alluded to. In connection therewith Professor Putnam cannot refrain from a mild indulgence in his favorite *manie*, hinting at the discovery of "several facts pointing to Asiatic arts and customs as the origin of those of the early peoples of Central America." (Shade of Brasseur de Bourbourg!)

With like enthusiasm, though on a less scale, the Department of Archæology of the University of Pennsylvania sent several explorers to the field in 1892, and has added largely to its collections by their efforts; while the National Museum, the Bureau of Ethnology, and the Smithsonian Institution will show in time by their reports that the year was also singularly fruitful for them.

The Study of Hair.

A note which I inserted in *Science*, Nov. 4, on this subject led Mr. Mott, F.R.G.S., to send me a reprint of an article which he read some time ago before the Leicester Literary and Philosophical Society. The position he maintains has at any rate the merit of novelty. Arguing (not quite correctly) that the highest races of man are the hairiest, he maintains that this is the result of natural selection; that, therefore, these hairiest types will increase, while the more naked forms will be eliminated; "until in a few centuries men and women will be clothed with natural garments of fine soft fur"; and the occupation will be gone of both Parisian milliners and the "old clo' man!"

More practical are the observations, in the last number of the *Zeitschrift für Ethnologie*, on the prevalence of moustaches in

women, by Dr. S. Weissenberg. He was struck with their frequency in Constantinople, and on several occasions counted the number of visibly hirsute upper lips on the women between eighteen and fifty years of age whom he met in the streets. He found it to be about ten per cent of the total number, which he justly claims is a high rate. On reading his article, I made similar observations in the streets and stores of Philadelphia, and found the moustached women between the ages mentioned to be less than three per cent; but I attribute little value to this statement; for I happen to know that the depilatory "Rusma" has an active sale in the drug stores, and that more than one physician makes a profitable little specialty of destroying unsightly hairs by electro-puncture. These considerations interfere with ethnographic observations. I have noticed more moustached women in Madrid than in any other city. Dr. Weissenberg believes the Armenian women present this peculiarity most frequently. The Madridlenas will surely push them close. In negroes and mulattoes it seems quite absent.

Recent Craniological Studies.

Professor Sergi, of the University of Rome, has lately published two applications of his "taxinomic" method in craniology; the one to a collection of 400 skulls from Melanesia, the second to a smaller assortment from Sardinia. In the former, he begins by speaking of the "chaos" in the descriptions of the "Crania Ethnica" of De Quatrefages and Hamy, averring that nothing less than the thread of Ariadne could guide one in such a labyrinth. He then describes his own "method," and very prudently gives a vocabulary of the astonishing Greek descriptive terms which he has coined, a few of which were mentioned in *Science*, Feb. 24, 1893. The result of his study he claims to be the annihilation of the Papuas as a race, and he demands that the name be stricken from the ethnographic lexicon, as the alleged Papuas are a compound of many varieties, not confined to Melanesia, as the term is generally understood, but extending over Australia, many islands of Polynesia and Micronesia, and even to the Andaman Islands. A few only of these varieties are localized, as, for instance, the *hypsisthenoclitobrachymetopus stenocrotaphicus neocaledonensis*!!

Applying his method to skulls from Sicily and Sardinia, from a moderate number Professor Sergi defines thirteen varieties in the former isle, and eight in the latter, and intimates that this does not at all exhaust the types. He believes that by noting such types, "we can follow the migrations and diffusion of the varieties which have peopled Italy, and resolve many problems in anthropology and ethnology hitherto obscure." He regards the long narrow cranium as that most ancient in the two islands, and it is a form still common among the inhabitants.

However much we may admire Professor Sergi's enthusiasm and the nicety of his observations, it must appear evident to the unbiased observer that his results are open to serious questionings. I find that in any collection of skulls, whether from Melanesia, Sicily, or Sardinia, he discovers by his "method" a new type in at least every twenty; he adduces no evidence to show that these "types" correspond to any ethnic distinction, whether physical or psychical; he makes no effort to show negatively that these various types are not from children of the same parents and same lineage; nor that the same types may not be found in perfection among races the most distant and of no ethnic relationship. I am sure that some of the types he describes are as truly American as they are Sicilian or Melanesian. The conclusions arrived at by such reasoning are, I submit, like those of other authorities which he himself stigmatizes as "not merely incorrect but misleading."

An interesting point in the anatomy of the skull is discussed in a late number (July, 1892) of the Proceedings of the Berlin Anthropological Society. It is in reference to the frequency of that enlargement of the palate known as the "torus palatinus." It is present in three-fourths of the Slavic Poles, in about one-half of the Sibiric tribes, in about one-fourth of the American Indians and Europeans of Aryan race; while it is quite absent among Jews and Gypsies. What its ethnic significance is, if it has any, remains for future investigators to determine.

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THE EVOLUTION AND USE OF THE AFTERSHAFT IN BIRDS.

BY HUBERT LYMAN CLARK, PITTSBURGH, PA.

THE presence of an aftershaft on the contour-feathers of the body has long been recognized as a taxonomic character of some value in the classification of birds, but little, if anything, has been published regarding its history or use. Admitting that every part of the body either has some function, the exercise of which has tended to preserve and strengthen it, or else that, if functionless, it is gradually being atrophied, it is necessary, before the evolution of any organ can be followed through all its stages, to first discover if it has any function, and, if so, what it is. The probable history of the aftershaft can best be traced in this way, and so the first point to be settled is the question of its use. The primary function of feathers is the retaining of heat, and, while forming a non-conducting covering, it is still essential that the weight be as little as possible. As the feathers differentiated other functions, some becoming long and stiff for flight and steering and some taking on new shapes and colors simply for ornament, every change which made the coat of contour-feathers more compact was a distinct gain to the bird. If any feather with a well-developed aftershaft be examined, it will be seen that the smaller shaft lies exactly underneath the larger and its vanes are closely appressed to those of the main feather, thus practically doubling its thickness and increasing its heat-retaining power with the least possible loss of compactness. Now, on the other hand, if a feather is examined which has only a very small aftershaft, the latter is not closely appressed to the main shaft and adds almost nothing to its thickness or warmth. Exceptions will, of course, be easily found to these rules, but the fact remains that where the aftershaft is vigorous it gives plain evidence of adding warmth to the plumage, while it is clear that it can have no secondary function of ornament or locomotion. Another reason for believing that the aftershaft is functionally of no importance, except when an assistance to greater warmth, is found by examining the list of birds which lack it. They are as follows:—

1. Some Ratitæ (Ostriches, Rheas, Apteryx?).
2. Diomedinæ (Albatrosses).
3. Steganopodes (Gannets, Pelicans, Cormorants, etc.).
4. Lamellirostres (Ducks, Swans, Geese), except Flamingoes and some Ducks.
5. Columbæ (Pigeons).
6. Cathartidæ (American Vultures).
7. Striges (Owls).
8. Pandionidæ (Ospreys).
9. Cuculidæ (Cuckoos).
10. Alcedinidæ (Kingfishers).

Since there are known to science to day about ten thousand species of birds, only one-tenth of which are included in the above list, the absence of an aftershaft may certainly be considered exceptional. If it is functionally of any importance, why should it be wanting in the albatross, though present in the petrel? Or

wanting in many ducks and present in others? And indeed to assign it any function common to all birds except to those in the above list, while wanting in all of them, will be readily found impossible. That it may be the cause of greater warmth receives a proof of negative value from our knowledge that, while very large in all the other Ratitæ, it is totally wanting in those species which inhabit the open plains and deserts of the tropics, where it is not desirable to retain too much heat. Still further evidence appears in the facts that all of the birds which lack an aftershaft (except owls and pigeons) are supplied with a thick coat of down beneath the contour-feathers, and all of the groups except Striges, Lamellirostres, and a few Steganopodes are most largely represented in the tropics and warmer temperate countries. While much of this evidence is very general, some of it purely negative, it seems undoubtedly true that the aftershaft, when not serving as an additional heat-retainer, is wholly functionless.

The conclusion is now unavoidable that the aftershaft, if functionless, must, according to our original proposition, be undergoing a process of gradual atrophy. That such is the case admits of little doubt. It must, however, be kept in mind that its possible function as a heat-retainer is admitted, and in cases where this function has been sufficiently exercised, atrophy, if ever begun, has been stopped. Illustrations of this may be found all through the class, but two will be sufficient to show the point. In the Casuaridæ (Cassowaries) the aftershaft is of equal size with the main shaft, and its function is undoubtedly the same. It is practically a second feather, and, since compactness of plumage is of no advantage to non-flying birds, it has continued to exercise its function, and atrophy has never begun. In the Gallinæ, one of the oldest and most generalized groups, where compactness of plumage is very desirable, not only because it creates less friction in flight, but also because, being essentially ground birds, they are greatly exposed to cold and damp, the aftershaft is large and thick, but entirely different from the Cassowary's. Increasing compactness of the plumage has greatly modified it, but atrophy has not occurred because it still exercises to an important degree its function as a heat-retainer. In the Passeres, on the other hand, the condition of the aftershaft shows evident loss of function and consequent atrophy, being very small and weak. Perhaps in no better way can the degeneration of the aftershaft consequent on its loss of function be proven than by an examination of the feathers of the wing. As is well known, the chief function of the primaries and secondaries is no longer heat-retaining but locomotive, and they entirely lack an aftershaft in all flying birds. But this change of function has undoubtedly been brought about gradually, and on the elbow of the wing are several feathers, very slightly different from the contour-feathers, which grade by almost imperceptible differences into the fully-developed secondaries. If these feathers are examined in any bird with aftershafted plumage, as, for example, the ruffed grouse (*Bonasa umbellus*), a very evident aftershaft will be found on the smallest ones, but decreasing rapidly in size as the main shafts become flight-feathers, until, on the true secondaries, they are either wholly wanting or represented only by a slight meeting of the vanes.

It will be noticed that throughout the preceding argument, the assumption has been made that the aftershaft is a degenerated and not a recently evolved part of the feather. That is, that it was originally characteristic of feathers in general and its condition as known to us is worse than formerly, rather than that it is an acquired character, which never occurred where it is now wanting. The truth of this assumption must now be proven, or the foregoing statements are meaningless. The first reason to be offered for believing it to be a primitive part of the feather is found in the facts of its occurrence and development in the different orders of birds. In the Casuaridæ, which is admitted to be one of the very oldest families of modern birds, we find a very large aftershaft, indeed, as already said, it is practically one-half of the feather. In the other Ratitæ, in which it is wholly wanting, local causes, such as excessive heat, have destroyed its usefulness, and its loss is easily explained. Large aftershafts are also found in *Opisthocomus*, the Gallinæ, and the Penguins, all old and little specialized groups; while, on the other hand, in the

most highly organized birds, as the Passeres, the aftershaft is very weak, and in many peculiarly specialized birds, as the owls, American vultures, ospreys, and kingfishers, it is wholly wanting. The second reason for considering it primitive is the process of its development during the formation of the feather. It is needless to republish here the history of a feather's growth, but it may be well to call attention to one or two points. When the malpighian layer covers the feather-papilla, it would naturally be thinnest on the sides. The increased thickness above and below would cause greater pressure on the papilla along the median line on both surfaces, thus causing the grooves in which the rhachis and hyporhachis subsequently develop. Now, it is known that both these grooves occur in those feathers which have an aftershaft, and it is much more probable that, though now the upper groove is the larger, they were originally of equal size, than that the lower groove is a secondary development; because it is difficult to assign any possible reason for its ever beginning at all as a secondary characteristic. The foregoing facts give warrant to the following theory of the evolution and subsequent degeneration of the aftershaft.

Paleontology shows us that flight was an accomplished fact long before birds were evolved, and, since it requires tremendous muscular energy, it would be an obvious advantage to the hypothetical avian ancestor to decrease his weight and, at the same time, increase the non-conductability of his covering. When, therefore, feathers were first evolved from scales, the object in view was increase of heat-retaining power combined with decrease of weight. The most natural way of improving scales in this direction would be to make them thicker and, at the same time, hollow, and continued development along this line would result in making them more or less quill like. Then by dividing longitudinally and at right-angles to the axis of the body the number would be doubled without taking up any more space on the body, an obvious advantage. Constant subdivision, making them more adjustable, more coherent, and more compact, would finally bring about a condition very similar to that of the down-feathers of many birds especially in the first plumage. From this condition it is not difficult to trace the gradual development into a contour-feather in which shaft and aftershaft are of equal size, such a condition, in fact, as we find in the Cassowaries. But in this condition the feathers cause far too great friction with the air to admit of rapid flight, and so there came about the natural evolution of the more coherent, pennaceous feather with its comparatively smooth surface. But the natural curve of the lower half of this primitive feather was up and outwards and in direct antagonism to the down and inward curve of the main shaft, and so, being a hindrance to the required compactness, it gradually gave way and degenerated to its present condition. The rest of the story has already been told; how, where the aftershaft has adapted itself to its sole function as heat-retainer, it is still strong and useful, but in all other cases it is either wholly lost or on the rapid road thereto. Whether subsequent investigations and discoveries in paleontology and histology confirm this theory remains to be seen, but, for the present, it is at least plausible and open to few objections.

BRITISH STONE CIRCLES.¹

BY A. L. LEWIS, LONDON, ENGLAND.

No. 1. — Abury.

THE largest circle of stones in the world was that of which the remains — few when compared with the magnitude of the structure of which they formed part, but by no means inconsiderable in themselves — are to be seen at Abury, in Wiltshire. Abury village is six miles from Marlborough station (Great Western Railway); it occupies the site of the circles and is mainly built of fragments of the stones which composed them. The monument when complete consisted of a circle of one hundred stones (more

or less), of which thirteen still remain above ground and at least sixteen more are buried, some of these are of great size, more bulky than any at Stonehenge, but unshaped and without the cross-pieces which distinguish the latter monument from all other circles. The diameter of this circle was about eleven hundred feet, or eleven times that of the outer circle at Stonehenge; inside it were two other circles, — north and south, — both over three hundred feet in diameter. Dr. Stukeley considered that there was a smaller circle inside each of these, but there is now nothing remaining of them, and it has been doubted whether they ever existed. In the centre of the northern inner circle there were three very large upright stones, forming a "cove" or three sides of a square, of which the open side was toward the northeast, and of these stones two still remain, besides which there are now only three stones of the northern inner circle or circles and five of the southern, and a single stone, which Stukeley said stood in the middle of the latter, has long since disappeared. The total number of stones composing the inner circles, "cove," etc., was, according to Stukeley, eighty-nine.

The circles (and the greater part of the village) are surrounded by a deep ditch, outside which is a high embankment. Aubrey, the first writer who noticed this monument, made a very imperfect plan of it in 1663, in which he represented an avenue of stones leading down in a straight line to the present main road, near the River Kennet, and another avenue of stones leading from the end of it, also in a straight line, but at a right-angle, to a smaller circle on Overton Hill, near the line of large barrows which crosses the main road from Marlborough before it reaches the point where the road to Avebury leaves it. Stukeley delineated these as one avenue running in a curved line about a mile long between the great circles at Abury and the smaller one on Overton Hill, and thought that it represented a serpent, of which the Overton Hill circle formed the head, and the Abury circles some convolutions of the body, the tail being represented by another avenue, which left the great circles near where the church now stands, and curved away to the left, passing two large stones called the "long stones," which are still to be seen,² though of the rest of the alleged second avenue nothing remains *in situ*, so that some archaeologists think it never existed, especially as Aubrey, who visited the circles more than fifty years before Stukeley, has not left any notice of it. Stukeley, however, spent much more time at Abury than Aubrey did, and obtained much information from the inhabitants as to the former position of stones which had been destroyed within their remembrance, and, as there is much stone used in causeways, etc., over the marshy ground on that side of Abury, it is probable that an avenue of some sort did formerly exist there, but this a point for the visitor to investigate for himself.

The circle on Overton Hill and the end of the avenue adjoining it were destroyed before Stukeley went to Abury, but there are several stones of the other part of the avenue standing and fallen by the side of the road which leads from the main road at West Kennet to Abury village, and in a meadow under the left-hand hedge of the main road there are four fallen stones of the avenue, and, as these follow the curve which the road makes between the barrows and the turn to Abury, they seem to show that Stukeley was right in delineating a single curved avenue in place of the two, meeting at right angles, which Aubrey shows in his plan. This is another point for the visitor to verify, and he will do well to follow the avenue from these four stones to its junction with the circles at Abury, and, having inspected the latter, to go out past the church to the "long stones," and to the Beekhampton Inn, which is on the main road by which he will return to Marlborough, stopping on his way to climb Silbury Hill, the largest artificial mound in Europe. This attracts attention by its regular shape and flattened top, and, as it is due south from the circles at Abury, probably formed part of the monument; it has been dug into, but nothing has been found to show it to be a sepulchral mound, like the smaller barrows which are so numerous in this district. Human remains were found round the Overton circle, but none are known to have been found at Abury, so that it does not appear that the object of these circles was, as some suppose,

² These are probably the last survivors of another large circle.

¹ It has been thought that many Americans who, when in England, visit Stonehenge may not be aware how many remains of a similar character, which they might also wish to inspect, exist in the British Isles; and the editor of *Science* has accordingly made arrangements for a series of short articles, which shall give a description of each of the principal circles and state what points should be noted and how it may most easily be visited.

the commemoration of the dead; but the fact that the "cove," or holy of holies, in the centre of the northern circle, faced the sun when rising at midsummer has been regarded as indicating sun-worship to have been the chief purpose of this vast monument, which was in all respects so suitable for a place of assembly for a tribe or nation.

A short distance to the north of the main road from Marlborough to Abury are the remains of a dolmen called the "Devil's Den," and there is another at Rockwell, four miles northwest from Marlborough and two miles northeast from Abury. There was also a circle at Winterbourne Bassett, four miles north from Abury, but it is not worth the trouble of a visit, as only three or four stones remain.

PHYSIOLOGICAL CONTRIBUTIONS FROM MISSOURI BOTANICAL GARDEN. I.

BY J. CHRISTIAN BAY, MISSOURI BOTANICAL GARDEN, ST. LOUIS, MO.

THE PLANT CELL.

IN the early part of this year, Professor von Sachs, of Wuerzburg, published a paper on the theory of cells: *Beitraege zur Zellen-theorie* in "Flora," 1892, Heft 1, pp. 57-64. The leading thought of this publication seems to me to form, when combined with the following suggestions, the key and basis for deductions from the very long and interesting series of facts which forms the results of investigations of the later years in the functions of vegetable cells, both mechanical and physiological.

It is not difficult to trace how, even since the epoch of natural philosophy ("die Naturphilosophie"), the science of vegetable physiology has been in want of a solid foundation, a base, upon which the results of investigations in the phenomena of the life of vegetable cells could be firmly built. In the *Botanische Zeitung* a lance was in vain broken for the old theory; somebody then in vain put out the question, what Schleiden would give us instead of the old natural philosophy. Schleiden made no answer, because he had none to give.

The physiology of the plant cell having had since that time no leading exponent is, I suppose, the reason why at present that science merely consists of a series of very interesting, suggestive facts, but without the necessary conjunction with regard to points of view leading to general results.

A great many prominent men have devoted their lives to the study of vegetable cells, and we must allow that botany has now progressed as far as zoölogy, but only with regard to the accumulation of facts, in animal biology the cellular physiology of Virchow, dating from 1858, has arrived at a very high stage of development. Therefore, when thinking of the construction of a comparative physiology of animals and plants, it will be a most thankworthy task to collect all of the thrown facts concerning the physiology (qua biology) of the plant cell and arrange them from a general point of view.

The reason why the botanical part of cellular science has not brought forth general results during this long period is also to be sought in the definition of the cell body in botany. Very few physiologists would allow that the plant cell as well as the animal cell is an organism. Still this definition is to be looked at as a necessary foundation for a clear perception of the phenomena of botanical cellular physiology, both mechanical and chemical. As far back as 1848, one of the most prominent physiologists, N. Pringsheim (*De forma et incremento stratorum crassiorum in plantarum cellula observationes quaedam novae*. Halae, 1848, p. 38.) reminded us that "cellula est individuum," Hilger and Husemann, Weiss, and A. Zimmermann have told us almost the same, but still we find such definitions as "Grundorgan" (Frank), "Elementargebilde," "Formelemente" (G. Haberlandt). In his excellent "Lectures," Vines calls the plant cell "the physical basis of life." It must be remembered that Huxley ("Physical Basis of Life") only spoke of the protoplasm as the bearer of life. And Huxley himself, when he gave this most ingenious definition, did not see in protoplasm the *physiological basis of life*. Life never rested on a physical basis, nor consisted in physical matters alone.

Nobody will doubt whether a yeast cell is an organism or not.

Professor R. Pedersen, of Copenhagen, for six years my teacher in physiology, first mentioned these facts to me in the winter of 1891, acknowledging the results of this consideration for the evolution of cell theory in botany. Never this explanation was said with regard to the fact that said definition subsequently would form the key to cellular physiology in botany and, I may add, to comparative physiology of animals and plants.

The question is of considerable importance, because the accumulated facts now need a basis. The proposition of Sachs in his recent paper must be said to have come in due time. Yet it evidently ought to be connected with the given definition of the cell. Now we shall be able to arrange the facts in a system, see where vacant spaces may be, and fill up the voids, but up to the present time we were unable to do so.

Taking the "energids" as a basis of vegetable life, Sachs found "a real unity as a basis for the plant body," when we allow an energid to be "one nucleus with that protoplasm which surrounds it and which is commanded by the same nucleus." Then, looking forward, we shall see as one of the necessary results that the cell, often containing more than one nucleus, is really an organism, never an organ. Even without this deduction we may acknowledge the cell as an organism, because it acts as an organism.

Mechanics not being life, life is not mechanics; physiology alone is the science of the functions of life. Therefore, to understand the latter we must find a good physiological foundation for it.

By this explanation I hope to have been able to show that investigations in the life of the plant cell ought to be brought into another trace in the future. More than usual plant physiologists must be aware that they want—as Sachs says—"a scientific language, according to the true scientific idea."

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

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An Alleged Mongoloid Race in Europe.

ALTHOUGH it is not usual, and often impracticable, for writers to reply directly to the various criticisms passed upon their books, yet, as an interested reader of *Science*, I may perhaps be allowed to say some words with regard to a review of my "Testimony of Tradition," contributed to your issue of Feb. 10 (p. 82), which I have not had an opportunity of seeing until to-day. This I desire to do in order to remove more than one misapprehension of my meaning in the work reviewed.

"The very slender basis for the whole theory," says the reviewer, "is the syllable *Fin*." In this he is greatly mistaken. Linguistic comparisons in this direction are certainly made, and considerable stress laid upon them, but these are entirely subsidiary to the important statements quoted in the first chapter. Briefly, these are to this effect: Wallace, a clergyman in Orkney during the second half of the seventeenth century, states that "Finn-men" were at that time occasionally seen off the coasts of Orkney, each "Finn-man" being the solitary occupant of a small skiff. In particular, he specifies the years 1682 and 1684, and another writer (Brand), who confirms his account, gives instances in or about the years 1700 and 1701. Their skin-boats, and the dress and usages of the people themselves, as described by these writers, identify them at once with Eskimoes, i.e., an Eskimo-like race. Of this there can be no reasonable doubt. Both writers state that one of their skin-boats was then preserved "as a rarity" in the Hall of the Edinburgh College of Physicians, and it is added that another specimen was preserved in the parish church of Burray, Orkney. The former statement is confirmed by an entry of the year 1696 in the minute-book of the Edinburgh College of Physicians, which I copied from the original writing and published in my book (p. 10). The writer first quoted (Wallace)

is stated — in quite another connection — to have been noted for his “veracity;” and this is quite borne out, in this instance, by the evidence of two contemporary writers and the minute-book of the College of Physicians.

That these skiffs were “kayaks,” and that their occupants were practically Eskimoes, is what I have never seen called in question. The first writer (Wallace) conjectures that they actually *were* Eskimoes from Davis Straits. This also is the explanation offered by some critics, whose preconceived notions prevent them from entertaining the idea that certain European castes, within comparatively recent times, may have been (ethnologically) Eskimoes. Wallace’s son, editing his father’s book in 1693, thinks it “a little unaccountable how these Finn-men should come on this coast,” from so great a distance as Greenland. And Brand (1701), who calls them also “Finland-men,” regards it as “strange” and “wonderful” that they should come even from Finland, — assumed by him to be their home. Orkney tradition, which styles them “Finns” and “Fin-folk,” connects them with a certain island in Orkney, with Shetland, with Norway, with the Faroe Islands, and even with Iceland.

It is perhaps within the bounds of possibility that Greenland “kayakers” made their way to the Orkneys, *via* Iceland, the Faroes, and Shetland, about the year 1680. But this assumption seems to me so unreasonable that I cannot entertain it. Were the “Finn-men” of 1701 the immigrants of 1682, and had they been living in retirement about the Orkneys all that time? Or, when pursued by the Orkney fishermen, as they often were, did they retreat on each occasion to Greenland? But it is futile to suggest questions such as these. It is much more reasonable to assume that the stories about “Finns” and “Fin-folk” (though blended in modern times with impossible stories about seals) have an actual historical basis, and relate to a people whose home was in Europe and not in America.

I dare not trespass further upon your valuable space, or I would say more with regard to the various points selected for criticism. I shall only add that the reference to the Delaware Finns of the seventeenth century is not my own, but is *quoted* (at p. 36); that “Finn” and “Lapp” were once used interchangeably, though now distinct; and that, according to C. F. Keary (“The Vikings,” p. 157), the Scandinavian peninsula, almost as far south as the 60th parallel, “was Lapp or Finnish territory” in the ninth century, which allows of possible surviving remnants at a much later date in that region, — not to speak of the British Isles.

DAVID MACRITCHIE.

Edinburgh, March 7.

A Possible Source of Confusion as to the Origin and Character of Certain Shells.

It is quite possible that in studying the fossils of a single stratum of rock or even so small a fragment as a hand specimen, one may find examples over which he pauses. Wide divergences may exist between shells that lie side by side. They have evidently been deposited from the same waters. Apparently, they have flourished under like surroundings of depth and character of water. Yet one example may bear traits of fresh-water origin, while another may be as distinctly of marine growth.

The key to the anomaly may probably be found in what is now going on along our lake shores. Take as illustration the intermingling of marine and lacustrine forms on the borders of Lake Champlain. In favorable places there are found closely packed accumulations of unios and related shells. The waves that have brought these to the shore have at the same time been gnawing at the banks of clay of the Champlain epoch. In these are imbedded saxicava and associated forms. The clay is worked over by the waters; the finer particles drift out into the lake, the coarser with the liberated shells sink down among the unios. So a firm stratum is made from forms now existing in the waters and those that long ago flourished there. These deposits await the phenomena that have consolidated like ones along shores in older geological time, after which shells of different origin and character may be broken from the same rock.

HENRY M. SEELY.

Middlebury, Vt.

BOOK-REVIEWS.

A Microscopic Study of Changes due to Functional Activity of Nerve Cells. Reprinted from the *Journal of Morphology*. By C. F. HODGE. Boston, Ginn & Co.

THE present investigation is the beginning of a new line of research, and Professor Hodge is to be congratulated on his successful pioneer work. It consists of an account of a long series of patient observations made upon the spinal nerve-cells of the frog and the cat under the influence of stimulation through the spinal nerves. The general conclusion is that stimulation of the nerve-cell produces changes, in the structure of the cell, which are visible to the microscope. The most noticeable and tangible of these changes is the shrinking of the nucleus. This shrinking of the nucleus was seen in all of the experiments described, and that it was not a pathological change was proved by the fact that a rest after the stimulation caused in a few hours a recovery of the nucleus to its normal size. Perhaps the most interesting results of the whole series of experiments was a comparison of the nerve-cells of the spinal cord and brain in animals killed in the morning after a night’s rest, and similar animals killed at night after a day’s activity. In every case a very striking difference in the microscopic appearance of the nerve cells was manifest. The whole line of work is extremely suggestive and very promising of important results in the future.

The Naturalist on the River Amazons. By HENRY WALTER BATES. With a memoir of the author by Edward Clodd. Reprint of the unabridged edition. New York, D. Appleton & Co. 395 p. Map. 8°.

AMONG the thousands of volumes that crowd the shelves of our great libraries there are few that have ever reached the honor of a second edition. Fewer still attain a third and fourth, and rare indeed is the instance of one that, decade after decade, and generation after generation, continues to delight the human soul. The vast majority of printed books are as ephemeral as the May-fly, born and dying in the same hour, read and forgotten as we read and forget the gossip of a Sunday paper. Those volumes that, no matter how often they are reprinted, are always fresh and new, and which give delight to the younger as they did to the older generation, we christen “classics.” Some have come to us from ancient Greece and Rome: others from the Middle Ages: some from more recent days. In no single century, however, are there more than a small number that ever reach the pinnacle of public approval and become designated as classics. The more books there are the greater the numbers that are cast aside; so that in our time, when thousands of volumes are being poured from the press year after year, the chances that any one will be successful in achieving the highest honor are slight indeed. A book must possess more than usual worth: give to the jaded world some new ideas, and be couched in language to be read by old and young with equal pleasure. Books like the one at present under review belong to that category which includes such volumes as White’s *Selbourne*, Darwin’s *Voyage*, and Wallace’s *Malay Archipelago*, — books which have fulfilled the requirements of classics, and which have been accorded that title by a grateful public.

No one can err, we believe, in placing Bates’s “*Naturalist on the River Amazons*” among the foremost books of travel of this age; and no one who has read it, but recalls its graphic pages with delight. Pages that bring to those who have not seen with material eyes the wonders of the tropic zone, images of delight; and that recall to those who have seen these wonders visions of never-to-be-forgotten pleasure. It is said of the ornithologist Gould, who had long desired to visit the forests of the Amazons, that, meeting Bates after the appearance of his book, he exclaimed: “Bates, I have read your book; I have seen the Amazons!” It is now thirty years since the first edition appeared. Since then many others have been printed, mainly based, however, upon the second edition. This, upon the advice of his publisher and to his lasting regret, Bates abridged to a considerable extent. The public is, therefore, most grateful to have reproduced, as in the beautiful volume before us, the unabridged words of the author,

written when time had not dimmed nor distance distorted the vividness of the images he depicts. But the volume also contains a sketch of the life of Mr. Bates, with extracts from his journals and letters, together with an abridgment of the celebrated article on mimicry in butterflies, which placed its author at once in the front rank of philosophical naturalists. The book, therefore, becomes practically a new one, which every lover of such literature should not fail to possess.

The life of Mr. Bates, aside from his travels on the Amazons, was an uneventful one. He was born in Leicester, on Feb. 8, 1825, and was apprenticed at the age of 13 to a hosiery manufacturer. He inherited from his mother a tendency toward dyspepsia, and so was always far from strong, but he early manifested a love for natural history, and spent all his spare time hunting butterflies and beetles. He began to write out his notes at an early age, and before he was 20 years old he had published some of his observations. It was while at Leicester that he made the acquaintance of Wallace, and the two friends went, in 1848, to the Amazons. His companion remained four years, but Bates himself stayed for seven years longer, and returned to England in 1859, with about 15,000 species, 8,000 of which were new to science, and with a wealth of observations that occupied his pen for many years.

Bates was the first to point out the curious fact of mimetic analogies between various species of butterflies, and to suggest that the cause of the mimicry was natural selection. He also suggested that the reason for the mimicry lay in the unpalatableness of the mimicked species, so that the mimickers, although themselves edible, escaped their enemies by taking on the form and coloration of the species that birds would not eat. An abstract of this paper, with a colored plate, is given in Dr. Clodd's memoir.

Among the more interesting portions of this part of the volume are a few extracts from Bates's journal. It is to be regretted that there are not more. In the following he records some of his impressions of Lyell.

"Sir Charles Lyell has the appearance of a fidgety man not well at ease with himself. He is very greedy of fame, and proud of his aristocratic friends and acquaintances. He does not seem to be a very ready man; his learning does not appear to be at his fingers' ends; so that when a subject is suddenly presented to him he has difficulty in collecting his scattered thoughts and bringing forth what he knows upon it. But then he is getting an old man now. Mr. Davidson told me he was a very hesitating writer, and re-wrote every sentence three or four times on the average. . . . But, like a well-bred gentleman, Sir Charles can become very sociable, and evidently likes a good dinner with brilliant conversation: Darwin says he likes to hear himself talk. At the Geological Club . . . he made me laugh by retelling a very good thing. The conversation ran on the comparative merits of the scientific hypothesis of the origin of man and the biblical man. 'Why,' says he, 'the question resolves itself into few words: Is man modified mud, or modified monkey?'"

It was due to the urgency of Darwin that Bates began and continued to write his travels; and on the appearance of the book in 1863 it met with cordial praise from all quarters. His style is direct and concise. While many writers would have given a long account of the outward voyage from England, Bates disposes of it in three lines, and plunges almost at once into the luxuriant forests that were to be his home for eleven long years. His first walk in the forest was taken with Wallace on the day of their arrival at Pará, and a part of it is thus described:—

"As we continued our walk the brief twilight commenced, and the sounds of multifarious life came from the vegetation around. The whirring of cicadas, the shrill stridulation of a vast number and variety of field-cricket and grasshoppers, each species sounding its peculiar note; the plaintive hootings of tree-frogs,—all blended together in one continuous ringing sound,—the audible expression of the teeming profusion of nature. As night came on, many species of frogs and toads in the marshy places joined in the chorus; their croaking and drumming, far louder than anything I had before heard in the same line, being added to the other noises, created an almost deafening din. This uproar of life, I afterwards found, never wholly ceased, night or day; in

course of time I became, like other residents, accustomed to it. It is, however, one of the peculiarities of a tropical—at least a Brazilian—climate which is most likely to surprise a stranger. After my return to England, the death-like stillness of days in the country appeared to me as strange as the ringing uproar did on my first arrival at Pará."

The fact of a struggle for existence among animals is generally recognized by all, but the same struggle among plants is not so easily observed. In the luxuriant forests of the tropics the fact is forced upon all observers, and Bates gives a striking example of it. A parasitic tree occurs very commonly near Pará, which has received the appropriate name of the Murderer Liana or Sipó. It is described and the fact commented upon as follows: "It springs up close to the tree on which it intends to fix itself, and the wood of its stem grows by spreading itself like a plastic mould over one side of the trunk of its supporter. It then puts forth from each side an arm-like branch, which grows rapidly, and looks as though a stream of sap were flowing and hardening as it went. This adheres closely to the trunk of the victim, and the two arms meet on the opposite side and blend together. These arms are put forth at somewhat regular intervals in mounting upwards, and the victim, when its strangler is full-grown, becomes tightly clasped by a number of inflexible rings. These rings gradually grow larger as the murderer flourishes, rearing its crown of foliage to the sky, mingled with that of its neighbor, and in course of time they kill it by stopping the flow of its sap. The strange spectacle then remains of the selfish parasite clasping in its arms the lifeless and decaying body of its victim, which had been a help to its own growth. Its ends have been served—it has flowered and fruited, reproduced and disseminated its kind; and now, when the dead trunk moulders away, its own end approaches; its support is gone, and itself also falls.

"The Murderer Sipó merely exhibits, in a more conspicuous manner than usual, the struggle which necessarily exists amongst vegetable forms in these crowded forests, where individual is competing with individual and species with species, all striving to reach light and air in order to unfold their leaves and perfect their organs of fructification. All species entail in their successful struggles the injury or destruction of many of their neighbors or supporters, but the process is not in others so speaking to the eye as it is in the case of the Matador. The efforts to spread their roots are as strenuous in some plants and trees as the struggle to mount upwards in others. From these apparent strivings result the buttressed stems, the dangling air-roots, and other similar phenomena. The competition amongst organized beings has been prominently brought forth in Darwin's 'Origin of Species'; it is a fact which must be always kept in view in studying these subjects. It exists everywhere, in every zone, in both the animal and vegetable kingdoms. It is doubtless most severe, on the whole, in tropical countries, but its display in vegetable forms in the forest is no exceptional phenomenon. It is only more conspicuously exhibited, owing perhaps to its affecting principally the vegetative organs,—root, stem, and leaf,—whose growth is also stimulated by the intense light, the warmth, and the humidity. The competition exists also in temperate countries, but it is there concealed under the external appearance of repose which vegetation wears. It affects, in this case, perhaps more the reproductive than the vegetative organs, especially the flowers, which it is probable are far more general decorations in the woodlands of high latitudes than in tropical forests."

It is as much in the reflections that the varied phenomena under observation give rise to as in the descriptive portions that the value and charm of the book lie. There is always something new. Now it is the colossal trees, then the wonderful profusion of insect life, or the graphic pictures of free life in the forest. Nothing is more striking than the difference between the fauna on the two banks of the great Amazons, and Bates refers to this in numerous places. So, too, the wonderful fact that certain insects, especially the butterflies, mimic others is of vast interest. Then the great variation presented by some forms of insects, so that at the two ends of a series we have what are commonly called distinct species, while there are intermediate forms presenting every gradation between them. Those naturalists who

claim that species do not originate *now* in a state of nature, would certainly find it a difficult task to refute or explain on any but one hypothesis the facts given by our author. One case in particular is of great interest. It would appear that two butterflies, species of *Heliconius*, are extremely abundant in the forests along the river. They inhabit, however, different sections of the country, one of which is moister than the other. One species occurs in the dry forests, the other in the moister ones. One species, *H. melpomene*, is black with a large crimson spot on its wings; the other is *H. theliopoe*, in which the wings are beautifully rayed with black and crimson and have a number of bright-yellow spots. Both have the same habits, and they have long been regarded as perfectly distinct species. We quote now Mr. Bates's words: "There are, as might be supposed, districts of forest intermediate in character between the drier areas of Obydos, etc., and the moister tracts which compose the rest of the immense river valley. At two places in these intermediate districts, . . . most of the individuals of these *Heliconii* which occurred were transition forms between the two species. Already, at Obydos, *H. melpomene* showed some slight variation amongst its individuals in the direction of *H. theliopoe*, but not anything nearly approaching it. It might be said that these transition forms were hybrids, produced by the intercrossing of two originally distinct species; but the two come in contact in several places where these intermediate examples are unknown, and I never observed them to pair with each other. . . . These hybrid-looking specimens are connected together by so complete a chain of gradations that it is difficult to separate them even into varieties, and they are incomparably more rare than the two extreme forms. They link together gradually the wide interval between the two species. One is driven to conclude, from these facts, that the two were originally one and the same; the mode in which they occur and their relative geographical positions being in favor of the supposition that *H. theliopoe* has been derived from *H. melpomene*. Both are, nevertheless, good and true species in all the essential characters of species; for, as already observed, they do not pair together when existing side by side, nor is there any appearance of reversion to an original common form under the same circumstances."

We have already so far overstepped our space that we must reluctantly refrain from quoting further. We would, however, call particular attention to the account given of Termites on pages 209-214; that on Fire Ants on page 227; on Monkeys on pages 331-345; and on the general features of ant life on pages 355-363. The remarks upon floating pumice on pages 263-264 are well worthy the consideration of students of geographical distribution, and those on page 169 are commended to the student of comparative philology, as indicating a method of the formation of dialects among savage tribes.

One word more and we have done; for even at the risk of tiring the patient reader we add one more quotation. After a life of eleven years spent in the Amazonian forests, certainly Bates was well qualified to judge between that life and civilized man's. He had formed a love for the country, and he took leave of it with regret. The desire, however, of once again seeing his parents and of enjoying the pleasures of intellectual society drew him from this "Naturalists' Paradise." "During this last night on the Pará River," he says, "a crowd of unusual thoughts occupied my mind. Recollections of English climate, scenery, and modes of life came to me with a vividness I had never before experienced during the eleven years of my absence. Pictures of startling clearness rose up of the gloomy winters, the long, gray twilights, murky atmosphere, elongated shadows, chilly springs, and sloppy summers; of factory chimneys and crowds of grimy operatives, rung to work in early morning by factory bells; of union workhouses, confined rooms, artificial cares and slavish conventionalities. To live again amidst these dull scenes I was quitting a country of perpetual summer, where my life had been spent, like that of three-fourths of the people, in gypsy fashion, on the endless streams or in the boundless forests. I was leaving the equator, where the well-balanced forces of nature maintained a land surface and climate that seemed to be typical of mundane order and beauty, to sail towards the North Pole, where lay my home, under

crepuscular skies, somewhere about fifty-two degrees of latitude. It was natural to feel a little dismayed at so great a change; but now, after three years of renewed experience of England, I find how incomparably superior is civilized life, where feelings, tastes, and intellect find abundant nourishment, to the spiritual sterility of half-savage existence, even if it were passed in the Garden of Eden. What has struck me powerfully is the immeasurably greater diversity and interest of human character and social conditions in a single civilized nation than in equatorial South America, where three distinct races of man live together. The superiority of the bleak north to tropical regions, however, is only in its social aspect; for I hold to the opinion that, although humanity can reach an advanced state of culture only by battling with the inclemencies of nature in high latitudes, it is under the equator alone that the perfect race of the future will attain to complete fruition of man's beautiful heritage, the earth."

JOSEPH F. JAMES

Text-Book of the Embryology of Man and of Mammals. By DR. OSCAR HERTWIG. Translated by Professor E. L. Mark. New York, Macmillan & Co. \$5.25.

PROFESSOR MARK has done a great service to English science by translating this text-book of embryology. The appreciation of the book abroad is shown by the fact that the edition which is now translated is the third edition since its original publication in 1866, the third edition of the first part of the text-book being demanded before the second part was ready for publication. The valuable scientific researches of Hertwig are very well known by all naturalists, and his name alone is sufficient to indicate the reliability of the work in hand.

The title, "A Text-Book of Embryology of Man and Mammals," does not adequately express the scope of the book, for while it is in details largely confined to the study of mammals, there is so much of general embryology within its covers as to give it a value as a general text-book of vertebrate embryology. As such a text-book it is of the greatest value to a student, and it is safe to say that at the present time there is no text-book so well designed to give the student a general knowledge of vertebrate embryology as the present one.

The subjects treated comprise all matters of importance connected with invertebrate embryology. They are treated in a masterly style, and the facts and discussions are in all cases brought up to date. In the chapters on the sexual products and their fertilization may be found a summary of the essential facts of our present knowledge upon this important subject. The chapter on cleavage discusses the general matter of the segmentation of eggs, giving the various types of such segmentation, their relations to each other and defining the terms used in descriptions in various text-books. The chapter on the gastrula is especially valuable, for it gives in a clear, logical, but concise manner, illustrated by valuable and intelligible drawings, our present ideas as to the application of the gastrula theory to the embryology of vertebrates. It is a subject which is always puzzling to the student of embryology to understand the gastrulation of the vertebrate egg, and Professor Hertwig has done very much toward making this difficult subject intelligible. Not the least valuable part of this section is an outline history of the gastrula theory, tracing our knowledge of embryology of the germ layers from its infancy to the present time. The gastrula theory is accepted by Professor Hertwig in its fullest sense. The chapter on the formation of the body cavity gives Professor Hertwig an opportunity of explaining clearly his "coelomthorie" which he does in a clear style, and the significance of which he makes plain by its historical consideration. In addition to the above, there are considered in the first part of the work the segmentation of the vertebrate embryo, the origin of connective tissues, the method of formation of the external form of the vertebrate body and a study of the foetal membranes of reptiles, birds, mammals, and man. In all of these sections the aim of Professor Hertwig is not only to give facts but to give a logical connective account of the significance of the facts and a logical understanding of the various phases in the development of the vertebrate body, and he has greatly added to the value of

the discussions by short historical accounts of the development of our knowledge on the various topics.

The second part of the book is devoted to the development of special organs, and here the author is more confined to the mammals and gives less consideration to the other vertebrates. The method of consideration is that of the study of organs according to their origin in the different layers of the body. These are considered, therefore, under four heads. The organs of the endoderm include the alimentary system in general; the organs of the ectoderm include the nervous system; the organs of the middle germ-layer include the muscles, the urinary and sexual organs. Professor Hertwig's views of the body cavity lead him to the formulation of a fourth layer of the vertebrate embryo, which he calls the intermediate layer or mesenchyme, and the last section of the text-book studies the development of the organs from this mesenchyme. These, according to Hertwig, are the circulatory system and the skeleton.

The special merits of this book are the logical treatment and its consideration of the embryological facts as parts of a system. The general method of the treatment of the subject is a comparative rather than a physiological one, and the text-book will give the student an insight into comparative anatomy but very little consideration of the physiology of the developing embryo. In one of the two sections, it is true, the mechanics of development are considered, but in general, the text-book is a morphological rather than a physiological study. This is, of course, a natural outcome of the line of work in which Professor Hertwig has been so successfully engaged for so many years.

Not the least valuable part of the book consists in the abundant literature. Some fifty pages are devoted to giving the titles and references to the most important papers of vertebrate embryology. The book has, also, another feature, somewhat rare in German scientific books, but of extreme value to students, in the form of short, logical, but intelligible summaries at the end of every section giving in outline the important conclusions.

On the whole, the text-book of Professor Hertwig is probably the best general study of vertebrate embryology that has appeared in the English language up to the present time, and it can be most heartily recommended to all interested in these subjects.

Chemical Lecture Experiments. Non-Metallic Elements. By G. S. NEWTH, F.I.C. London and New York, Longmans, Green & Co. 323 p. 8°.

A BOOK of chemical lecture experiments, carefully classified and systematically arranged, cannot but be welcome to many. Moreover, a book from a practised hand, such as Mr. G. S. Newth, chemical lecture demonstrator in the Royal College of Science, South Kensington, has a particular value in that its experiments are so given as to be readily repeated and are not, as is often the case, merely a statement of the reaction with a few confusing details. Mr. Newth has chosen his experiments well and has described them in clear concise language. The book has a two-fold purpose in easing the labors of the lecturer and of the student alike. For the former it supplies a useful repertoire of lecture experiments and will surely be gladly received, removing, as it does entirely, the humdrum search for such examples and reactions as can be suitably and successfully demonstrated on the lecture table. This, as every lecturer knows, is by no means a small item in the preparation of a lecture, and, moreover, being important, it cannot be carelessly or hastily done. The experiment must be quickly and successfully performed or the interest of the student is turned to illy-concealed ridicule, and the lecturer is, so to speak, lost.

To the student the book appeals in providing a ready reference to serve as a companion in the lecture room, and in supplying the deficiencies of his notes. Indeed, it may in most cases entirely relieve him of the necessity of taking notes upon the experiments themselves, drawings of the apparatus, etc., and he will thus be enabled to devote his attention to the explanations

CALENDAR OF SOCIETIES.

Anthropological Society, Washington.

March 14.—Major John W. Powell, A Study in Psychology.

Geological Society, Washington.

March 22.—G. K. Gilbert, An Open Fissure; G. P. Merrill, Remarks on the So-Called Onyx Marbles or Travertines; C. D. Walcott, The Algonkian Rocks of the Grand Canyon of the Colorado.

Chemical Society, Washington.

Feb. 9.—W. H. Krug, A New Method for Estimating Furfurol-Hydrazone; E. E. Ewell and H. W. Wiley, On Some Products of the Cassava Plant. Professor Wiley describes the plant as it occurs in Florida, and says there is every reason to believe that, if the attention of capitalists is called to it, a large quantity of land now covered with pines could be profitably cleared and devoted to the cultivation of the cassava plant. A minimum average yield is four tons of roots per acre, which may be readily increased by proper fertilization to eight or ten tons per acre. Maize could not compete with cassava if the same intelligent cultivation is applied, and there is a prospect that the cassava will eventually take the place of maize in the production of starch, glucose, etc.

March 9.—W. D. Bigelow and K. P. McElroy, Determination of Lactose in Presence of Invert Sugar and Sucrose.

Philosophical Society, Washington.

March 18.—W. H. Holmes, Traces of Glacial Man in the Trenton Gravels; Asaph Hall, The Planet Mars.

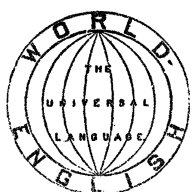
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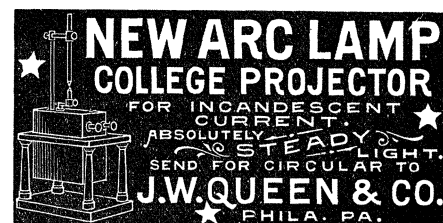
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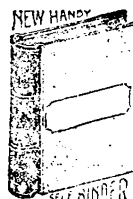
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and arguments of the lecturer. For the further aid of the student, equations are given representing the various reactions described, and every reasonable detail of the operation is recorded. Not only for the student in the lecture room is the book valuable, but also for the many who have not the advantage of college demonstration and who are compelled to rely largely upon their own resources. For the convenience of such teachers as may not have ready access to books of reference, Mr. Newth has added in the form of an appendix a number of important and useful tables, thirty-four in all, not often found in the smaller works on chemistry.

C. P.

An Introduction to Qualitative Chemical Analysis by the Inductive Method. A Laboratory Manual for Colleges and High Schools. By DELOS FALL, M.S. Boston and New York, Leach, Shewell & Sanborn. 8°.

IN this age of text-books it is difficult to be original, and the most that our authors have aimed at is perfection and revision of arrangement. Mr. Delos Fall, M.S., Professor of Chemistry at Albion College, has, however, recently given to the student of chemistry a new manual of qualitative analysis decidedly unique in its character. As he asserts in his prefatory note, this manual is designed to impart but little chemical truth directly, aiming rather to lead the student to gain that truth himself as nearly as possible at first hand and as a product of his own thinking. A preliminary chapter intended for both the student and the instructor gives the general plan of the book and describes the method to be used. This consists essentially in leading questions calculated to bring the student on by his own research from the fundamental ideas of chemical theory to the more advanced practical application of his knowledge to systematic qualitative analysis.

The plan as described by the author is a combination of (1) original investigation and (2) reference to authorities. "In the beginning (1) will be a very small factor and (2) correspondingly

large. As experience in manipulation, observation, and interpretation increases (1) will increase and the necessary dependence on (2) will decrease. To the experienced chemist (1) is very large and (2) very small; in other words, he is his own authority." Under the guidance of an able instructor the book will be most valuable, and in such a case will, as the author has himself demonstrated, produce accurate, enthusiastic, and independent students.

C. P.

The Batrachians and Reptiles of the State of Indiana. By OLIVER P. HAY. Indianapolis, William P. Burford, Printer and Binder.

THE present work consists of a list of the reptiles and batrachians found in the State of Indiana, with a description of their characteristics and with analytical keys for the determination of species. The work describes 81 species in all, and is accompanied by a few plates illustrating the subject. The design of the author is to make a key which shall be usable by those who are not specialists, and he has therefore appended a glossary, explaining the use of all scientific terms, and his general method of treatment is such as to make the book intelligible even to a novice.

The Birds of Indiana. By AMOS W. BUTLER. From the Transactions of the Indiana Horticultural Society for 1890.

THIS little pamphlet consists simply of a list of the birds found in Indiana, either as residents or as temporary migrants. No characteristics of species are given, although a large number of illustrations are inserted, taken from Coues's "Key to North American Birds."

THE price of Dr. Sternberg's work on bacteriology, reviewed in *Science*, Feb. 24, should have been stated as \$8, cloth; and \$9, sheep; Wm. Wood & Co., New York, publishers.

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For sale, or for exchange for books on medicine or surgery, new editions only, a large geological library, containing nearly all the State and Government Reports since 1855. Will be pleased to answer letters of inquiry and give information. Address R. ELLSWORTH CALL, Louisville, Ky.

For exchange.—Slides of Indian Territory Loup Fork Tertiary Diatoms for other microscopic fossils. Address S. W. WILLISTON, Univ. of Kansas, Lawrence, Kans.

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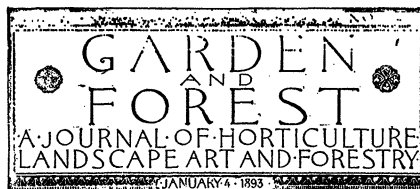
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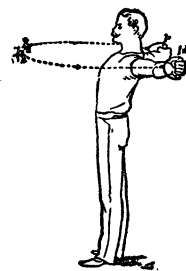
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